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The participants in this conference (A.V. Cicourel, I. Deutscher, S. Ervin-Tripp, J. Gumperz, D. Hymes, E. Sibley, H. Phillips, and R.B. Anderson) discussed the processes involved in eliciting verbal information, especially in cross-cultural studies. The four principal topics of their discussions were: (1) the ethnography of asking questions, (2) the meaning of words--particularly as influenced by social and linguistic contexts, (3) the use of linguistic and social data from research on the preceding two topics in developing theories and methods in the several disciplines, and (4) the need to correct some shortcomings of current research. It was felt that "we simply do not know how to phrase questions that will be meaningful to random samples of diversified populations." The participants agreed that almost all contemporary social science research would be improved by much more careful attention to ethnographies of interrogation, and that analyses and interpretations based on current research are often highly problematic. (JD)

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LANGUAGE AS OBSTACLE AND AS DATA IN SOCIOLOGICAL RESEARCH

by Allen D. Grimshaw *

LINGUISTIC and other intergroup differences in communicative behavior, previously regarded simply as obstacles to cross-cultural research, have lately come to be increasingly recognized as an important but hitherto largely unexploited kind of sociological data. Under the auspices of the Council's Committee on Sociolinguistics, these two aspects were explored at a small conference of linguists and social scientists held in San Francisco, November 25–26, 1968.

One of the reasons for the conference was the desirability of discussion of the fact that linguistic factors narrowly defined are closely intertwined with other factors conditioning communication. The participants therefore undertook broader consideration of the process of eliciting and interpreting verbal information, touching, for example, on the problem of categorizing "don't know" responses to questions and on different codes of polite behavior as pitfalls in the search for comparable data.

The participants in the conference included anthropologists, a psychologist, and sociologists, more than half of whom had been trained in linguistics.[1] Four principal topics were discussed: (1) the ethnography of asking questions; (2) the meaning of words—particularly as influenced by social and linguistic contexts; (3) the use of linguistic and social data from research on the preceding two topics in developing theories and methods in the several disciplines; (4) the mobilization of educational and research resources to correct some shortcomings of current research which were believed to be a consequence of failure to incorporate knowledge about the preceding topics into contemporary scholarship. It was generally agreed that investigators in these areas ought to be persuaded that some of their "incidental findings" are necessary building blocks for general theories of communication and other social behavior.

What follows is only a sampling of the range of subjects and ideas covered in the course of the conference, with no attempt to assign credit for specific ideas. A more extensive record of the conference may be available at a later date.

ETHNOGRAPHY OF ASKING QUESTIONS

Middle-class white Americans are accustomed to answering questions, to being interrogated by family, friends, and strangers; to being tested and measured, surveyed and polled; to filling out forms. As one participant suggested, "They carry around responding skills

* The author is Professor of Sociology, Indiana University. As a member of the Council's Committee on Sociolinguistics he was responsible for organizing the conference on which he reports here. He wishes to thank Aaron V. Cicourel, Irwin Deutscher, Susan Ervin-Tripp, John J. Gumperz, and Dell Hymes for extensive critical commentary on drafts of this report but observes that, since the participants in the conference could not always agree, the report (which here has been much condensed for reasons of space) can be considered neither a completely accurate record of the meeting nor a definitive statement of the problems and prospects of an area of scholarly activity. Thanks are also due Kathleen George for bibliographic and editorial assistance.

[1] Committee members in attendance included Susan Ervin-Tripp, Allen D. Grimshaw, John J. Gumperz, Dell Hymes, and Elbridge Sibley, staff. The other participants were R. Bruce Anderson, Duke University; Aaron V. Cicourel, University of California, Santa Barbara; Irwin Deutscher, Case Western Reserve University; and Herbert P. Phillips, University of California, Berkeley. Several other scholars provided copies of pertinent unpublished papers for use at the conference. Most of the participants have had field experience in which they employed formal questionnaire techniques, supplemented by participant observation. (The field sites represented include Argentina, Denmark, India, Norway, Thailand, Northwest and Southwest American Indian communities, and a variety of subcultural settings in the United States.) This experience enabled participants to judge communication problems by taking the points of view of native informants as well as of social scientists.

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so that they can accommodate their responses to the response categories available." Our questions, however, may be variously insulting, threatening, humorous or boorish, or simply meaningless to participants in other cultures (or subcultures in our own society). Or questions we deem legitimate may go beyond the bounds of propriety, legitimately expected knowledge, or even hypothetical contemplation—to others. There are, as a matter of fact, societies in which questioning behavior among unrelated adults is not tolerated or where it is an egregious insult to ask an adult to repeat a statement. In many societies, moreover, only individuals to whom certain roles are assigned, such as mothers or priests, can legitimately provide information about their own roles. On the other hand, Garfinkel observes that a culture may *require* persons to talk about, label, or be able to report certain kinds of behavior or events.[2]

It is often assumed that there are three quite discrete types of sentences—declarative, interrogative, and imperative—which serve respectively the social functions of giving information, asking questions, and issuing commands. But that assumption can be incorrect in any society and in some it frequently is. Furthermore, agreement as to what grammatical form serves what intended behavioral consequence varies in different communities and, within communities, with the context of the speech event. In polite American middle-class society some interrogative sentences are variously intended as requests, polite instructions, or undeniable demands. Some questions, of course, are rhetorical; and such a sentence as "My, isn't it cold in here?" is variously interpreted as a statement, a question, a command to close a window, or simply as a time-filling and contentless ritual. Questions can be asked about retrospective, immediate, or anticipated events—possible, unlikely, or even impossible; about one's own behavior or that of others; about one's own attitudes or ideas, or those of others. Examples of these sorts of questions are:

1. "When did you learn to read?" (about a probable actual event)
2. "What would you do if your six-year-old child ran away?" (about a possible hypothetical event)
3. "What would you do if you were President?" (about an unlikely [for most people] hypothetical event)
4. "How satisfied are you with your life?" (about a personal attitudinal set)
5. "How do you feel about the *X* party?" (about an ideological set)
6. "How are things going?" (nonquestion ritual).

This list is not exhaustive. These and other types of questions are asked by English-speaking natives of the United States. Not all these types of questions are asked of all adults in all societies; some are asked of no adults in some societies; or some types are expected to be asked by some adults and other types by others.

Questions are asked in social contexts, by and of interlocutors with different social characteristics, and via utterances that can be coded linguistically. Standard treatises on interviewing warn about the dangers of cross-class and cross-subcultural interviews, but they do not adequately consider the fact that there are respondents who cannot (in their own culture) legitimately be expected to have opinions or information on certain items. They do not mention the strains generated when standard speakers who consider dialects to be "obscene" interview those who speak them, nor discuss the equally disastrous consequences of spuriously and incorrectly imitating lower-status speech. In view of these oversights it is not surprising that such research manuals give little, if any, attention to the ways in which sequencing and other behavioral strategies identify sentences as statements, requests, questions, and so on. Finally, and particularly in prescribing techniques for schedule and questionnaire construction, they seldom take account of the fact that interrogative sentences are embedded in a context of other sentences which may influence the meanings intended and perceived. In short, they attend neither to the realities of differentiated systems of interrogative behavior nor to the necessity of developing theories for the adequate and correct interpretation of utterances.

Such a theory would concern, minimally, social norms for the maintenance of interpersonal boundaries in discourse—defining the proper initiation, carrying on, and termination of interrogative interaction. Are there, for example, what Harvey Sacks has called "tickets" for the initiation of interaction with strangers? What linguistic or other violations disrupt interaction once initiated? In our society interaction may be disrupted if an interlocutor demands explicitness when shared meanings should be assumed, or offers it when it implies that the other person has not the wit to understand. Thus far, only a few ethnographic semanticists have begun to work on systematic ways of finding out ethnographically how to ask proper questions.[3] One outcome of the con-

[2] Harold Garfinkel, *Studies in Ethnomethodology*, Englewood Cliffs: Prentice-Hall, 1967.

[3] As, for example, when Duane Metzger and Gerald E. Williams state ("Some Procedures and Results in the Study of Native Categories: Tzeltal 'Firewood,'" *American Anthropologist*, April 1966, pp. 389–407) that they wish to identify "classificatory differences significant to informants rather than to the investigators." See also their "A Formal Ethnographic Analysis of Tenejapa Ladino Weddings," *ibid.*, October 1963, pp. 1076–1101. For another approach see Charles O. Frake, "The Diagnosis of Disease among the Subanun of Mindanao," *ibid.*, February 1961, pp. 113–132, and "Notes on Queries in Ethnography," in A. Kimball Romney and R. G. D'Andrade, eds., "Transcultural Studies in Cognition," *ibid.*, June 1964, Part 2, pp. 132–145.

ference is heightened awareness of the importance of an ethnography of interrogative behavior for each and every social research project, and of the ultimate need for a general theory of universal rules for obtaining information.[4]

MEANING OF WORDS, PHRASES, SENTENCES, AND SETS OF SENTENCES

Several kinds of differences of meaning are highly valuable as social data as well as being an obstacle to understanding. There are, of course, difficulties of varying degree in translating single words across languages; most undergraduates know about the problems of finding equivalents for *verstehen* or *sympatico*. But within a single language community there can be quite different meanings for the same word—meanings change over time (even within the span of a single interaction), across subcultural groups, and also as a result of shifts in topic or setting in interaction involving only members of the same subgroups. Even single words within languages and within cultural subgroups (or even stable dyads) can have wide ranges of both cognitive and expressive meanings. In a superb study of the Russian novel Friedrich has shown that both objective information on status relations and very profound subjective expression were conveyed by the selection of one or another personal pronoun.[5] In English, which does not have the *tu-vous* distinction, we are all familiar with how the meaning of the address term "Sir" can vary with social context and, in a given social context, with linguistic context, intonation, and so on. These problems are subtly different from those of alternative denotations of a given word, e.g., *chair* in "I had the chair."

The problem of meaning has been attacked mainly by three methods: use of the semantic differential,[6] back translation,[7] and componential analysis.[8] The first of these is the technique that has been most widely used in attempts to isolate the "meaning" of individual words—to identify their qualities, the emotional and subjective connotations of strength or weakness, warmth or coldness, friendliness or unfriendliness. The semantic differential is not used to look for the cognitive boundaries of terms, nor to specify how the same word, when embedded in different sentences or used by different people in different social settings, may have quite different expressive meaning. Interest appears to be in the individual words themselves and their differentiation. There has been little concern with applications of this technique either in the improvement of cross-language or cross-cultural social research or in the use of results of such analysis for answering sociological questions.

Clyde Kluckhohn, in his delightful *Mirror for Man*,[9] presents a piquant example of back translation. The phrase "Genevieve suspended for prank" is translated into Japanese; it re-emerges in English, "Genevieve hanged for juvenile delinquency." We are not told where the error appeared nor how Japanese readers would have understood Genevieve to have been punished. The Londons, in their examination of the questionnaire administered to refugees in the Harvard Project on the Soviet Social System, have shown how the use of one word, *kar'era* (career), with unanticipated negative implications in the Soviet context, resulted in highly questionable findings.[10]

Back translation presents some obvious problems of reliability.[11] Some of them can be reduced by successive iterations by different translators. More satisfactory reliability, however, provides no assurance that questions are valid—initially, in translation, in retranslation, or in interpretation.[12] Standard stimuli simply do not exist—and this is as true of research across class or color boundaries in the United States as of research across languages or societies. In part, this is because questions do not exist in isolation; every question is linked to every other question, and immediately adjacent questions may have quite different influences in

[4] For some beginnings in this direction see Aaron V. Cicourel, "The Acquisition of Social Structure: Towards a Developmental Sociology of Language and Meaning," in Jack Douglas, ed., *Existential Society*, New York: Appleton-Century-Crofts, in press.

[5] Paul Friedrich, "Structural Implications of Russian Pronominal Usage," in William Bright, ed., *Sociolinguistics: Proceedings of the UCLA Sociolinguistics Conference, 1964*, The Hague: Mouton and Co., 1966, pp. 214–259. For more general treatment see Roger Brown and Albert Gilman, "The Pronouns of Power and Solidarity," in Thomas A. Sebeok, ed., *Style in Language*, Cambridge: Technology Press, and New York: John Wiley & Sons, 1960, pp. 253–276.

[6] Charles E. Osgood, George J. Suci, and Percy A. Tannenbaum, *The Measurement of Meaning*, Urbana: University of Illinois Press, 1964; Charles E. Osgood, "Semantic Differential Technique in the Comparative Study of Cultures," in A. Kimball Romney and R. G. D'Andrade, eds., *op. cit.*, pp. 171–200.

[7] Herbert P. Phillips, "Problems of Translation and Meaning in Field Work," *Human Organization*, Winter 1959–60, pp. 184–192.

[8] See, e.g., Ward H. Goodenough, "On Componential Analysis and the Study of Meaning," *Language*, January–March 1956, pp. 195–216, and Eugene A. Hammel, ed., "Formal Semantics," *American Anthropologist*, October 1965, Part 2.

[9] New York: McGraw-Hill Book Company, 1949.

[10] Ivan D. London and Miriam B. London, "A Research-Examination of the Harvard Project on the Soviet Social System," *Psychological Reports*, December 1966, Part 2, pp. 1011–1109.

[11] For an early discussion of some of the theoretical problems of back translation, see Susan M. Ervin, "Information Transmission and Code Translation," in Charles E. Osgood and Thomas A. Sebeok, eds., *Psycholinguistics*, Bloomington: Indiana University Press, 1965, pp. 185–192 (originally published in 1954).

[12] For a timely commentary on reliability and validity, see Irwin Deutscher, "Looking Backward: Case Studies on the Progress of Methodology in Sociological Research," *American Sociologist*, February 1969, pp. 35–41.

different languages or cultures. If the semantic differential has been limited because it has been directed to individual words, the technique of back translation is limited if it is applied only to single sentences. It is clear that we need to know more about how sentences are linked—and interimplicated. Perhaps linguistic analysis should transcend sentence boundaries. There are clues here both for the creation of more valid research instruments and for more intelligent and more broadly productive interpretation of answers to questions asked in social research.

Attention at the conference turned to ways in which componential analysis might be extended to include more specifically sociolinguistic concerns, and the techniques adapted to problems of research across social categories. The terms used in componential analysis—domains, sets, dimensions, components—have clear and simple definitions: domain refers to natively relevant contrasts; a set consists of all those words that legitimately can be substituted in a phrase or sentence; dimensions and components have their usual meanings. The methodology of componential analysis is quite straightforward. Its purpose is to isolate natively relevant contrasts and to identify relationships among different usages—to uncover the patterned character of cultural systems—whether systems of linguistic behavior or of kin labeling. In the case of the identification of complex components (or features), a considerable amount of ethnographic work may be involved. In attempting to obtain a usable definition of the extended family in India, for example, it is necessary to ask several questions of a number of people in various places. In one area a shared hearth may be of cultural importance; in another, the sharing of complex and complementary ritual obligations; in still another, financial aid to even very distant kin may be salient. Whatever local specialization there may be, however, some common core of explicit connotations and implicit understandings can be discerned deeply embedded within the patterns of local variations. The aim of componential analysis is the specification of this common core.

This specification can be valuable to the sociolinguistically oriented scholar in three ways. First, through laying bare the mutual implications of cultural meanings and linguistic usages, it provides an approach to the complex causal relationships between language structure and social structure—between speech behavior and other social behavior.[13] Second, it provides data for simple studies of linguistic behavior and its social structural correlates, in which speech usages can be analyzed, for example, to identify class characteristics, without concern over causal priority. Finally, by showing that cultural meanings cannot be taken, semantically uncontaminated, out of either componential domains or social contexts, it can help us both to ask questions and to interpret responses more intelligently. Componential analysis may seem, to the uninitiated, to be arcane. It is not magical. It will not enable the investigator to ask questions, across languages or cultures, which are simultaneously phenomenally identical and conceptually equivalent. It will indicate sources of error and it will permit a probing into valid meanings which has not characterized prior research.

Partly because every question is related to all other questions, standard interpretations of similar responses are hazardous: all responses in an interrogatory interaction are linked. It may be possible to localize individual questions and responses so that variable conditions and effects can be isolated. We have no evidence that this is true. We do have reason to believe that treatment of questions and responses that are similar in form as if they were semantically equivalent is dangerous indeed.[14]

ETHNOLOGY OF ASKING QUESTIONS AND SOCIOLINGUISTIC DATA

To almost any question some respondents answer "don't know" or some equivalent. Typically, such responses are excluded from cross-tabulations, with or without mention of this exclusion in the explanatory text. A somewhat more sophisticated practice is to identify those categories of respondents who frequently answer "don't know" across items and to characterize them, collectively, as "ill-informed" or "noncooperative." There are few attempts to discern patterns in which some respondents answer "don't know" to one set of items and others, to a different set.[15] There is seldom an attempt to discover whether there may be

[13] For a review of some perspectives on these causal relationships (adapted from Dell Hymes) see Allen Grimshaw, "Sociolinguistics," in Wilbur Schramm *et al.*, eds., *Handbook of Communication*, Chicago: Rand McNally & Company, in press.

[14] During the conference Aaron Cicourel was asked what differences there would be in results if he were to redo the fertility studies by Kurt Back and J. Mayone Stycos or those by Judith Blake, using his research techniques, or if they were to do studies in the Argentine, using their own. His response was that there would probably be little difference in the tables produced, but he believed he would have a different understanding of the *meaning* of entries in the tables, and more information on how to link demographic studies to more general theories of social interaction.

[15] Robert E. Mitchell ("Survey Materials Collected in the Developing Countries: Sampling, Measurement, and Interviewing Obstacles to Intra- and Inter-National Comparisons," *International Social Science Journal*, 1965, pp. 665–685) identifies differences between Chinese minorities and indigenous groups in Southeast Asia by comparing questions on which differential frequencies of "don't know" and "no

some items, for example, which elicit "don't know" from respondents of high socioeconomic status who would seem to be better equipped to respond, while those less well equipped give a substantive answer. A straightforward "don't know" may reflect unwillingness to go beyond *known* information, or it may mean something quite different. But all too frequently such responses are not recognized as a rich source of data, nor are systematic attempts made to reduce the ambiguities in research design which may have elicited them.

Whether working with survey data or with more extensive clinical materials, social scientists like to have "clean" data. They tend to forget that the interview itself is a part of the data. Instead, they aim to eliminate the variability of human response in order to standardize data. Homogenization, however, entails a loss of information. Procedures through which data are collapsed into manageable numbers of categories should be made explicit, so that lost data can be recovered and findings can be better interpreted.

Social scientists, who have long been concerned primarily with variables, have recently begun to turn their attention to constants, searching for possible social interactional equivalents of linguistic universals. It may be that universal patterns of behavior will be discovered through the identification of some social analog to the deep structure of language. Likewise, a componential analysis of interaction as it is revealed by speech may provide clues to persistencies and uniformities in human exchange. Without specifying how this search might most profitably be conducted, the participants in the conference agreed that it should be pursued by studying language behavior.

PROGRAMMATIC CONSIDERATIONS AND ADDITIONAL QUESTIONS

The participants agreed that almost all contemporary social science research would be improved by much more careful attention to ethnographies of interrogation, and that analyses and interpretations based on current research are often highly problematic. There was less agreement on the most effective way to introduce to practicing scholars and the generation of students now being trained the kinds of considerations suggested in the preceding pages—and to induce them to change their ways. The observation was made, more than once, that "establishment social science" has a substantial investment in doing things as they are now done (i.e., not treating current practices as data to be studied, per se), and that demands for the specification of how data are collected and interpreted would be regarded in some quarters as *lèse-majesté* or, at best, a bothersome inconvenience.

Social scientists *must* learn to incorporate such specification into their work and to report the ethnographic procedures they followed in arriving at such specification. Reference has already been made to the need for study of the interview itself—including research on data resulting from the use of different types of interviews (and interviewers) in different kinds of communities. One suggestion made at the conference was that previous research, particularly survey studies, should be re-examined with a view to isolation of the sociolinguistic factors that produce different data from questions and procedures that are intended to be identical. Studies of characteristics of interviewer, respondent, translator, and coder as they interact to place one reaction to a query in one category and others in others are greatly needed.

We simply do not know how to phrase questions that will be meaningful to random samples of diversified populations. We suspect that fixed-choice questions should never be used in comparative studies. Since those who use them have not systematically examined the possibly resulting biases, however, we have no way of estimating the magnitude or direction of errors that are thereby introduced. Preliminary ethnographic work might reduce the possibility of asking needlessly sensitive and hurting questions, but we have not learned to define studied populations as publics. We state (in discussions of collaboration with indigenous scholars) that access and understanding should both be aspects of research strategy. But we simultaneously make access problematic by failing to find out what the questions we are asking may mean in context, and deny ourselves understanding by considering single sentences in isolation in designing research instruments.

Social scientists must ask themselves some serious questions about how wisely they ask questions of their research subjects. Otherwise, continuing refinements in quantitative analysis of data will produce only spurious or at best marginal increments of socially and sociologically relevant data. Participants in the San Francisco meeting are convinced that questions can be improved and answers more intelligently interpreted.

answer" appear. This is rare, as is Mitchell's demonstration that some survey analysts are fully aware of the range of problems confronting cross-societal survey workers. Unfortunately, solutions to some of the problems are not so simple as he suggests.